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TELEPATHY AND EVOLUTIONARY THEORY

BY PROFESSOR A. C. HARDY, F.R.S.

IN my presidential address to the zoological section of the British Association for the Advancement of Science at its annual meeting in Newcastle-upon-Tyne last September (1949) I made a brief reference to telepathy. ' If it has been established, as I believe it has,' I said, ' then such a revolutionary discovery should make us keep our minds open to the possibility that there may be so much more in living things and their evolution than our science has hitherto led us to expect '. Earlier in that address I had expressed my conviction, on other grounds, that a mechanistic or materialistic interpretation, except merely as an account of the physical and chemical processes within the body, was quite inadequate for a true science of living things. In ending it I tried to bring the results of the experimental study of telepathy to the notice of those biologists who had not yet given them serious consideration ; I did so because I think it most likely that this research will very soon render a materialistic interpretation of life quite impossible.

It was gratifying that following this address the Society for Experimental Biology arranged an afternoon symposium on Paranormal Phenomena at their London conference in January 1950 at which papers were given by Dr E. J. Dingwall, Dr S. G. Soal, Professor C. D. Broad, and Professor J. B. S. Haldane, F.R.S. I don't think it was an exaggeration when, in opening that session as chairman, I said that the meeting might in a modest way prove to be an historic one. I believe this was the first time that any general biological society, certainly in this country and perhaps in any other, had devoted a session to the discussion of these matters ; thanks to the action of its Council in arranging the meeting, I am sure that many more biologists than hitherto are now taking an interest in the results of psychical research. That meeting may, I hope, have forged a closer link between these two branches of science.

In my British Association address I went rather further than just mentioning telepathy ; I felt it desirable to give an illustration of

the way in which our views on the process of evolution might possibly be altered by the acceptance of telepathy as a fact. It may be thought idle to speculate upon such matters at the present time when we know so little; yet if we feel, as I do, that the present generally accepted mechanistic theory of evolution cannot possibly be the whole truth, then I think we should have some hypothesis in mind so that eventually it may be tested by experiment or observation and, if rejected, replaced by another one. Such a hypothesis I very briefly tried to outline. From the questions I have since been asked it appears that I sketched it much too lightly so that many got a wrong impression of what I really meant. 'Give us some examples', they say, 'of the way in which the factors you have in mind could bring about evolutionary change'. Just when I was feeling that I should do so, the Editor of our S.P.R. *Journal* kindly invited me to contribute an article elaborating my ideas on this very subject; I gladly accept the opportunity.

DEVELOPMENT OF MODERN EVOLUTIONARY THEORY

To be sure of making myself clear I must give some account, if only a very bald statement, of the development of modern evolutionary theory; for it is to this that I must link the hypothesis. In giving this very brief history I must ask readers to excuse me for reminding them of much that they already know; I am doing so partly because the hypothesis I want to discuss relates to the theory at several points and also because I feel it is essential that I should make plain the evolutionary views which I hold. Ideas on evolution had, of course, been put forward before Darwin's time without finding general favour. It was Darwin's and Wallace's similar and independent simple explanation of the process, together with Darwin's great marshalling of evidence in his *Origin of Species*, published in 1859, that soon convinced the thinking world that evolution had taken place. The essence of the Darwinian doctrine is, of course, the principle of natural selection. Animals and plants tend to vary in all sorts of ways and some of these variations are inherited from one generation to another. They reproduce at such a rate that there is intense competition for available supplies of food—only a very small proportion can survive to maturity. Some varieties will be more successful in the struggle for life than others; they will tend to survive—to be, as Darwin said, selected by nature—and so to contribute more to posterity. The less efficient forms will tend to be eliminated and consequently appear less often in the ancestry of future generations.

This ruthless automatic mechanism of natural selection which appeared to govern evolution, together with the fact that men clearly appeared to be evolved from the lower animals, seemed to lead to a philosophy of materialism. Many who revolted against such a doctrine, while convinced that evolution had taken place, returned to the earlier doctrine of Lamarck, set out in his *Philosophie Zoologique*, published in 1809.

Lamarck's theory was that animals become modified in form by their own strivings and exertions—that *change of habit* gives rise to new needs, that the fulfilment of these new needs leads to the greater use of some parts of the body and disuse of others, that the body is thus developed or reduced in accordance with use or disuse, and that such acquired modifications are handed on, if only in some small degree, to the next generation. Such a theory, which suggested that the results of the endeavours of the individual were perhaps not entirely lost at death, but passed on, naturally appealed to idealists ; to many it seemed the last hope of reconciling the intuitive faith in the spirit of man with the fact of evolution.

Linked with the doctrine of Lamarck, but thought of quite independently, came the sparkling ideas of Samuel Butler with his concept of a gradually developing sub-conscious racial memory handed on from generation to generation. We find these views most fully expressed in his *Life and Habit* (1877) : animals, by repeating new actions, learn to perform them more readily—those at first made consciously and with difficulty are eventually performed unconsciously with ease, just as man so learns to skate or ride a bicycle ; not only, he thought, do individuals unconsciously remember how to perform such actions, but an unconscious memory of how to do them may be transmitted with the germ cells to future members of the race. There is, he postulated, not only a physical inheritance but an accompanying unconscious handing-on of memory ; in this way, he believed, newly acquired habits become converted into inherited instincts, and by the repeated satisfaction of these new desires, by new actions repeated generation after generation, the body becomes modified by the greater use of one part and disuse of another on Lamarckian lines. For him the whole functioning body and its form had in this way been built up in evolution bit by bit : the day-old baby sucks, digests, breathes, and so on, all most complicated operations, because its ancestors have done these things so often before—the knowledge of how to do them having become unconscious and automatic by age-long practice. Butler's conception was perhaps the logical development of Lamarck's idea, and those who follow him are sometimes spoken of as Neo-Lamarckians.

There are very few biologists today who subscribe to the Lamarckian doctrine; it has, indeed, for a long time been in eclipse. Each experiment that has been designed to demonstrate a Lamarckian effect appears to have failed; although it must be pointed out that very few Lamarckian experiments indeed have ever been performed. The mistake is very commonly made that the inheritance of the direct effects of the environment was part of Lamarck's theory of animal evolution; indeed, nearly all the so-called 'Lamarckian experiments' have been to test this very effect which Lamarck himself denied. 'Circumstances', he said, 'influence the form of animals. But I must not be taken literally, for environment can effect no direct changes whatever upon the organization of animals'.[1] The point he always made was that changes in the environment could bring about *changes in the habits* of animals. The direct effect of the environment as a cause of evolutionary change was, of course, the earlier theory of Buffon; this Lamarck seems to have accepted in regard to plants, but not for animals. It has often been said by Lamarckians that the effect to be looked for will be so small and take so many generations to show itself that we cannot hope to demonstrate it in any experiment carried out in a man's lifetime. To such an argument the modern Darwinian will reply that any such effect, if not Mendelian, could be of no value in evolution because it would be so quickly swamped by the effects of chance genetic variation and selection.

There is further a vast range of wonderful adaptations of animals —involving not only bodily structure but instinctive behaviour— which could only, according to present views, be produced by Darwinian selection. I refer to the remarkable instances where one animal obtains protection by mimicking in bodily structure and elaborate colour-schemes some other object—often another living animal—and, in the latter case, mimicking it in its behaviour as well as in bodily form, as when a spider may mimic an ant by lifting up and moving its front legs exactly like an ant's antennae. All these remarkable protective resemblances have meaning only if viewed *from outside—at some distance from the animal.* They

[1] A shortened translation of the following from Ch. 7 of the *Philosophie Zoologique* (Paris, 1873 edition), vol. 1, p. 223:

Ici, il devient nécessaire de m'expliquer sur le sens que j'attache à ces expressions: *Les circonstances influent sur la forme et l'organisation des animaux*, c'est-à-dire qu'en devenant très-differentes, elles changent, avec le temps, et cette forme et l'organisation elle-même par des modifications proportionnées.

Assurément, si l'on prenait ces expressions à la lettre, on m'attribuerait une erreur; car quelles que puissent être les circonstances, elle n'opèrent directement sur la forme et sur l'organisation des animaux aucune modification quelconque.

could not have been produced on Lamarckian or Butlerian lines unless the animal could continually strive to modify itself in front of a mirror! If Lamarckism fails here, among the most remarkable cases of adaptation, we have no right to suppose it works in the other cases of adaptation and evolutionary change.

Butler's ingenious theory of course broke down at several points ; it failed, for example, to explain the evolution of the kinds of instinctive behaviour which we see occurring in many animals only *after they have already laid their eggs* and so could not influence the offspring's subconscious memory : when, for example, a solitary wasp, having deposited its egg in an underground cell, catches a caterpillar, paralyses it with its sting, and brings it to serve as food for the future larva. It was likewise in great difficulties when trying to explain the instinctive behaviour of the neuter castes of social insects.

The Darwinian doctrine, after successfully overcoming two great difficulties, has gone on from strength to strength to its present triumphant position today. Both of these obstacles were due to an insufficient knowledge of the nature of variation and its inheritance. When the detailed study of variation by measurement was begun by Francis Galton towards the end of the last century, it was soon shown that nearly all natural objects varied in size, weight, shape, colour, etc., in a particular way. If we measure a fairly large random sample of any natural objects of the same kind, for example leaves, seeds, or whole animals of the same age, sex, and species, we shall find that there is a range of size covered by the whole sample. If we now count up the number of our objects for each different unit of size within the total range we shall find that there are very few of the extreme sizes, large or small, more of the less extreme sizes, and still more of those nearer to the average in size. Their different frequencies along the range of size, if plotted as a graph, will approach what we call a normal curve of error : a bell-shaped curve falling away from the mean on either side. They appear, in fact, to be varying according to the laws of chance. It was then thought that if we selected examples of our animal or plant of, say, larger size than the mean, and bred from them, we should find that their offspring would tend to vary in the same sort of chance way : some being slightly larger, some slightly smaller, and the majority nearer to the *size of their parents*. So it was confidently thought, at this time, that if we went on selecting for larger size, or some other character, generation after generation, we could go on pushing evolution in this or that direction as we liked within the limits of an efficient working organism. This seemed an obvious deduction because if variation was really quite a matter of chance then surely the

offspring must continue to range in size more or less equally above and below the size of their parents, even when by our selection we repeatedly chose the larger offspring to be the parents of the next generation. It was taken for granted that this indeed was what the stock breeder was doing in producing his different races of domestic animals. Yet each time this simple experiment in selection was tested in the nineties of last century it failed to give the expected result. Darwinism, indeed, appeared to have failed. It was not until 1909, from the work of the Danish botanist Johannsen, that we really understood why it had appeared to fail and also knew that the supposed failure was not in fact a breakdown of Darwin's theory. Johannsen clearly showed that organisms vary in two distinct ways: by the different effects of the environment (temperature, nutrition, etc.) which are not inherited, and by differences in their constitutional make-up which are in fact hereditary. The selection experiments which appeared to fail had been attempting unwittingly to select the former because at that time it was generally assumed that all kinds of variation would be inherited. Darwin's theory had not failed, however, because he had explicitly stated that to bring about evolutionary change natural selection must act upon variations which are inherited. We may remind ourselves that the full title of his great book was *The Origin of Species by means of Natural Selection or the preservation of favoured races in the struggle for life*; by the word *races* he clearly meant hereditary strains.

The other main difficulty of Darwinism in the last century was removed by the rediscovery of Mendel's laws of inheritance in 1900. Before then it was thought that there was a 'blending inheritance' in which the influence of one parent blended more or less equally with that of the other. This made it very difficult to imagine how in fact a new favourable variation could ever be successfully selected; for whenever such a new type occurred it would be most unlikely to find another of the same new type to mate with, so the result at once would be a dilution of the valuable new character by half, and then most likely by three-quarters, and further by seven-eighths in the next two generations. Few seemed to realise this consequence of blending; but those who thought about it saw what a very grave obstacle it was to the acceptance of Darwin's doctrine. Just at the turn of the century De Vries put forward his theory of evolution by mutation.

> The properties of the organism [he said] are made up of units sharply distinguishable from one another. These units are bound up in groups and, in related species, the same units and groups of units occur. Transitions, such as are seen in the outer forms of animals and plants, no more exist between the units than between the molecules of the

chemist. Species are not continuously connected but arise through sudden changes or steps. Each new unit added to those already present forms a step and separates the type as a species independent from the species from which it arises. The new species is a sudden appearance. It arises without visible preparation and without transitions.

Mendel's great discovery, neglected since it was first published in 1865, when brought to light in 1900 appeared at first to favour De Vries's theory and for a time Darwinism was in eclipse. For the first quarter of this century evolution was mainly thought of as proceeding by random mutations with selection playing quite an insignificant role. Actually, however, already in this period, the genetical research following on the rediscovery of Mendel's laws was gradually preparing the way for a grand re-birth of the Darwinian doctrine. Mendel had shown that inheritance was *particulate* and so the idea of blending inheritance, that shadow which must have haunted Darwin, was swept away.

Mendel's first law states that every animal and plant carries two of each kind of a large number of different unit hereditary factors —or genes, as we now call them—and that when the germ cells are formed, each such cell (e.g. egg or sperm) receives only one of each kind of factor or gene. The fertilization of the egg by the sperm from another individual restores the double number of genes to the representative of the new generation. As this new individual in turn becomes adult and forms germ cells of his own, the same process of the segregation of the two members of each pair of genes takes place. It is the united action of these hereditary genes which, together with the uninherited effect of the environment, govern the form of the body—and by governing the form of the nervous system may in turn govern instinctive behaviour and perhaps a predilection for various activities. From time to time slight changes in the genes occur—they mutate, as we say—so that in any population there are a number of different versions of each kind of gene. Thus, by the continual bringing together of different versions of these genes by the mating of different individuals there is a continual production of new variation upon which selection can act. We note that there is here no permanent blending of the units of inheritance or genes : however much they are shuffled together in inter-breeding, however much their *effects* are influenced by one another's presence, they themselves remain unchanged except when *very rarely* they are altered by mutation. A new valuable mutant gene is not blended away to next to nothing in the course of time ; it is passed on from generation to generation, in and out of different ' gene complexes ' without itself being changed.

It is quite impossible in a short article to give any adequate idea

of the vast amount of genetical and cytological work of this last half-century which has thrown so much light on the physical basis of inheritance; of how Morgan and his co-workers showed that the genes lie in the chromosomes of the nucleus of the cell—how they are reduplicated and evenly divided at each cell division and so spread unaltered to every part of the body during development—how they were found to be arranged in a particular linear order along the chromosomes and how in the maturing process of the germ cells (eggs or sperm) a remarkable mechanism of segregation, and chromosome breakage and recombination, is set in motion to bring about a maximum degree of re-assortment of different genes in the next generation.

We have seen, as the half-century has gone on, the idea of unit factor or gene governing a unit character of the body give place to the more modern concept of the gene complex. It is now known that not only may *one gene* have an effect upon *many different* parts of the body (and these effects may be both structural and physiological; for example, length of leg and the range of temperature the animal can tolerate), but all these *effects* may be *altered by the presence of other genes*; in fact, all the genes are acting together to create a joint internal genetical environment. This is what we mean by the gene complex.

Now, since the effect of any gene depends in some degree upon the united action of the other genes present, the effect of a particular new mutant gene may be more beneficial or more harmful in one gene complex than in another. This leads to the important concept that the *effect* of any gene is *subject to genetic variation*: it, *the effect*, may be altered by changes (mutations and recombinations) in the rest of the gene complex; thus—more important still—it, the effect, is therefore *subject to selection*. The effect can be altered by the continual selection of the gene complexes in which its effect is more favourable and by the elimination of those in which it is more harmful. So nicely balanced is the gene complex in relation to the external environment that only very rarely can we expect a new mutation to be an improvement: the vast majority must be detrimental and so eliminated. For the same reason it is the mutations *with very small effects* that are most likely to be successful in improving the stock; a big or violent change is most likely to be harmful.

How far we are now removed from the conception that De Vries put forward in *his* mutation theory; his so-called big mutations, which he had found in certain stocks of the evening primrose he was studying, were not ordinary mutations as we now understand them, but have since been shown to be highly exceptional hybrid abnormalities. Far from evolution proceeding as he thought by

big jumps—to him almost new species bounding into existence with selection playing no part in their production—we see tiny changes developing under the influence of selection acting upon the gene complex : little changes far below the species level of difference. We see—as Darwin had imagined—small inherited variations : some new and better ones being preserved more often than others in the struggle for life—and so gradually increasing their proportion in the population—while those that are less fitted to their environment are gradually eliminated from the race. We are back to Darwinism—the action of natural selection upon small inherited variations ; but it is Darwinism with a difference—an important difference—a Darwinism in which a *particulate* inheritance has been substituted for the old idea of blending. It is Darwinism and Mendelism united in one doctrine. It would be invidious in such a short account to attempt to pick out a few names from the many who have contributed to this modern position, but one name must be mentioned : no one has played a bigger part in bringing about this union than our mathematical geneticist, Professor R. A. Fisher, F.R.S.

It will be realised that in this bare outline of the development of evolutionary theory I have had to leave out much ; I have put in only what I feel to be necessary for the discussion I am now coming to. I may have given an impression that the course of evolution has been one long story of success ; but for every advance the fossil record shows that there have been very many failures : through hundreds of millions of years we see the stream of life repeatedly branching into paths leading to extinction. I have left out all mention of the recent evidence of the actual working of selection in nature and of the essential need of at any rate a temporary isolation of populations in the process of the splitting-off of new species from the older stock. I have not referred to the differences in effect that there may be between inter- and intra-specific selection, to sexual selection, to pre-adaptation, and to many other topics which could only be covered in a much longer article. One final touch, however, I must add to my sketch of the present position. It now seems likely, on physical and chemical grounds, that the genes are elaborate molecules and that their change, their mutation, is due to a change in the pattern of the arrangement of their atoms ; in support of this we know that the rate of mutation can be increased by irradiating animals with X- or gamma-rays.

To the majority of modern biologists, the outlook their science presents certainly appears to be a materialistic one. I fully accept the present position—as far as it goes ; but surely no one can

imagine it as anything like a final position. It is less than a hundred years since the publication of Darwin's masterpiece and only half a century since Mendel's laws were rediscovered. We have seen how in this short time our conceptions of the nature of variation and inheritance have undergone radical changes ; we must look for and, in time, expect to find very much more. ' It is a much-worn platitude to say ', as I said in my recent address, ' that it is nonsense to consider the works of a Shakespeare, a Beethoven, or a Rembrandt, as the product of a machine ; but it is a platitude worth repeating. If man is a member of the animal kingdom as we all believe, his works of genius are a manifestation of organic behaviour. To proclaim that organic behaviour is the product of mechanism as we *ordinarily understand it* is to my mind as unreasonable and as dogmatic as to proclaim the literal interpretation of Genesis '. It is also, I believe, extremely damaging to civilization.

TELEPATHY AS A POSSIBLE FACTOR IN MOULDING BEHAVIOUR PATTERNS

It seems to me most likely that there must be at least one element in the process of evolution that is not mechanical or material in the ordinary sense ; not simply because I am wishfully thinking it should be so, but because I believe it is unlikely that telepathy should suddenly have appeared from nowhere in the human organism. I am in this article assuming the reality of telepathy and also that the experimental evidence suggests that its occurrence cannot yet be explained in terms of physics as at present understood. The discovery that individual organisms are somehow in psychical connection one with another across space is, of course, one of the most revolutionary biological discoveries ever made ; so revolutionary that until very recently few biologists have regarded it as even a possibility worth investigating. Such a faculty—a property almost as fundamental as that of gravity between physical bodies—can hardly be peculiar to a relatively small proportion of one species of animal ; surely it is more likely that only a relatively few individuals are usually conscious of what is really a general property of organisms. If we admit that telepathy is established in man, then I think we must expect something akin to it—unconscious no doubt—to be a factor helping to mould the patterns of behaviour among members of a species.

The idea that there is something like telepathy acting between animals is of course not a new suggestion. Edmund Selous wrote a book *Thought-transference, or What, in Birds* (1931) on his observations on the wheeling flight of flocks of various birds and

suggested that the members of a flock acted in unison through such an influence. It may well be, of course, that there is from a leader a wave of movement through the flock which is too quick for the human eye to detect ; it is hoped that before long this may be determined by high-speed cinema-photography. The late Mr Whately Carington in his book *Telepathy* (1945) has a section on the subject ; after discussing the remarkable web-spinning activities of spiders and the difficulty of imagining the complex inherited pattern of nerve-paths in the brain to account for it, he writes as follows :

> I suggest that instinctive behaviour of this high order or elaborate type may be due to the individual creature concerned (e.g. spider) being linked up into a larger system (or ' common subconscious ', if you prefer it) in which all the web-spinning experience of the species is stored up.
>
> When I have occasion to tie up a parcel, I do not perform the requisite motions automatically, unless I have had long previous practice at tying parcels of very approximately that size and shape, as shop assistants have. On the other hand I equally do not approach each parcel-tying problem altogether *de novo* ; to a certain extent I am helped and guided by my memories of previous struggles, even though they may not come specifically and vividly to mind. Somewhat similarly, I suggest, the industrious spider may be to some extent aided by the accumulated content of what I suppose we might reasonably call the Spider Mind ; and *mutatis mutandis*, of course, in other cases. (p. 160).[1]

The way in which some such element as telepathy could possibly be involved in the evolutionary process might, I believe, occur through what is called Organic Selection, a discussion of which I have postponed until now. The gene combinations which are best suited to the *habits* of the animal may tend to survive in preference to those which do not give such full scope to the animal's pattern of behaviour. This idea, which was first put forward independently by Baldwin[2] in America and Lloyd Morgan[3] in this country at the turn of the century, had been almost forgotten until comparatively recently, when interest in it has been revived particularly in relation to habitat preferences rather than to marked structural changes. The conception of a possible selection of structural variations through change of habit, as opposed to the selection of other variations by the environment, is one of special interest for it is in effect a factor similar to that postulated by

[1] See footnote on p. 237.

[2] J. M. Baldwin, ' A New Factor in Evolution ', *The American Naturalist*, vol. 30 (1896) pp. 441 and 536 ; *Development and Evolution* (New York and London, 1902).

[3] C. Lloyd Morgan, *Animal Behaviour* (London, 1900) p. 115.

Lamarck, but brought about on true Darwinian lines. If, for some reason or other, an animal changed its behaviour—perhaps through coming upon some new kind of food—and now exercised different parts of its body from those it usually employed—say by digging for its new prey rather than by chasing it by running—then no one would doubt that those gene combinations which modified the fore-limb structure in the direction of more efficient digging would tend to survive more often than others less efficient. This is change of habit bringing about structural modification, but not simply and directly on account of the actual greater use and disuse of parts as Lamarck supposed, but through this particular form of Darwinian selection. It may be said perhaps that it is difficult to appreciate any real difference between such organic selection and ordinary natural selection, and that the reason why more attention has not been paid to it is, in fact, because most people feel that there is no really significant difference between them. The difference that I believe is worth noting is that between two elements in this natural selection ; on the one hand there is selection brought about mainly by the external environment—the action of predators and the rigours of climate, etc.—and on the other an internal or organic selection governed primarily by the habits of the animal. It is true that the habits of the animal are no doubt largely influenced by external factors—as, indeed, Lamarck said long ago, ' Changes of environment bring about changes of habits in animals '—but the fact that different patterns of behaviour may bring about a selection of different gene combinations may, I think, be of real importance.

To emphasize the difference in the two kinds of selection that I am trying to stress I will give again an imaginary illustration I used in my recent address. Man, by his selective breeding, can alter the form of domestic animals to suit, within limits, his own desires ; he selects those which better suit his needs. No modern biologist would doubt that if we knew as much about the genetics of man as we do about the genetics of some animals, then if mankind wished to control marriages by law, he could, by permitting some and prohibiting others, gradually in the course of long periods of time, alter the human race. Modern biology points to that, not as something desirable or undesirable, but as a theoretical possibility. If that is in fact a logical deduction from the present biological position, you will see where it must lead us. Evolution would no longer be guided from outside the species by natural selection, but by a directive activity from within the organism itself. We would see an organism directing its own evolution towards a goal in the future, whatever that might be decided to be.

In the hypothetical case just given, although man would be

carrying out conscious organic selection, he would still be subject to natural selection, by external agencies: for example, the elimination of certain gene combinations less resistant than others to the attacks of pathogenic organisms. There would be an interplay between the two selections: natural and directive. If it is logically possible in man to imagine an internal organic selection rivalling and dominating an external natural selection, it would seem to suggest that in other animals the interplay between the two kinds of selection may not be unimportant.

If there is to be expected, as has been suggested, something akin to telepathy, no doubt unconscious, acting between different members of a species and binding them together in a particular pattern of behaviour, then I think the fact that changes in behaviour can bring about the selection of different gene complexes would give evolutionary significance to such a group or species behaviour complex. That is, of course, provided that such a group behaviour complex gradually changed with the changing experience of the individuals making up the group.

There might be, as indeed the late Mr Whately Carington has suggested in the passage quoted above, a sort of species memory governing to some extent the pattern of behaviour of the individuals of the race. When I gave my British Association address I was not aware that he had made this suggestion or I should have acknowledged it then, although I don't think he intended to attach an evolutionary significance to it.[1] If there was such a non-conscious group behaviour plan, distributed between, and linking, the individuals of the race, we might find ourselves, as I said in my address, coming back to something like those ideas of subconscious racial memory of Samuel Butler, but on a group rather than an individual basis. Such a racial memory continuing outside the individual would, of course, remove the difficulties which were fatal to his hypothesis, such as the problem of the instinctive behaviour of parent insects after the laying of their eggs.

It may be argued that such a group behaviour plan linking the different members together might act against change of habit. This I think in part might well be so—we could expect it to be conservative; but also I think it possible to imagine this conservative plan being gradually altered by the influence of more enterprising individuals. Let us suppose a species of bird usually fed on insects off the surface of the bark of trees; then suppose

[1] When I got Mr Carington's book in 1945 I read the first half describing his experimental work but then, finding myself unable to follow his association hypothesis, I put it on one side. Had I read a little further I should have come to this passage to which Dr Soal has drawn my attention.

that, in a time of shortage, a few individuals discovered that they could find a good supply of insects by probing with their beaks into and under the bark ; they would be likely to be copied by other members of the species. The emergency of a food shortage in a particular area would compel a number to act against the conservative traditions of the racial behaviour plan ; if this action proved beneficial a wave of change in the behaviour plan might spread—not simply by copying, but perhaps by a telepathic-like influence spreading from the individuals who had made the new discovery. An inclination to explore below the bark might be transmitted. Now by organic selection change of habit would bring about change of structure—those whose gene combinations produced a slight change in shape of beak that was more useful in the probing of bark than the older type would tend to get more food and survive more easily.

Perhaps it might be suggested that primitive tribal religions are in part the development of such a racial behaviour complex ; but that, for the present, is venturing on to much too imaginative and dangerous ground. What I am here concerned with is to suggest that, if telepathy is established as I believe it is, then we must be prepared to consider its possible evolutionary implications. The only speculation I am making is a revival of Samuel Butler's idea in a new form ; it may no doubt be improbable, but I think its possibility is worth keeping in mind. If it should ever prove to be a fact, it would, as I said in the address that gave rise to this article, be a wedding of the ideas of Darwin and Mendel on the one hand and those of Lamarck and Samuel Butler on the other. The only excuse for such speculation is the hope of stimulating interest in the design of experiments to test whether anything akin to telepathy can be demonstrated to take place between animals ; I hope to make such an attempt when I have finished researches to which I am at present committed.

TWO PARALLEL DREAMS OF DEATH, AND THEIR SEQUELS

A Personal Experience Reported by Professor H. H. Price

On the night of September 19th–20th, 1949, I had a dream about going to bed out of doors in the afternoon. I seemed to be in the cricket ground of Magdalen College (on the north-eastern outskirts of Oxford). Towards the western edge of the cricket-ground,

twenty yards or so from its boundary, there was an ordinary-looking bed, oriented north and south, with an iron bedstead. At the same time the words ' three weeks before the shortest day ' were heard, though I was not aware of anyone speaking them. I have the impression, though I cannot be sure, that the rather curious description ' going to bed out of doors in the afternoon ' was actually given in the dream itself, though here again I was not aware of anyone speaking these words.

Either in the dream itself, or immediately after waking up, the following interpretation came spontaneously into my mind : the bed was a grave, and ' going to bed out of doors in the afternoon ' meant being buried. In my mind, as in many people's, there has long been a firm association, going back to childhood, between dying and going to sleep, and between burial and putting to bed or ' laying to rest '. As for the cricket-ground, I can only say that Magdalen is one of the three Oxford colleges of which I am a member, and that a cricket-ground is supposed to be a place of relaxation, and therefore perhaps of rest—at any rate, a place in which one does not work. But I was not much troubled about this point at the time, because the symbolism of ' going to bed out of doors ' appeared to me so transparently obvious.

It was curious that the date was given so exactly, and yet in an oblique or indirect way which reminds one a little of mediumistic communications. I assumed that the shortest day was the Winter Solstice, December 22nd, and that the predicted day for my burial was therefore December 1st. I also assumed that the year was the one in which the dream occurred, 1949. This is the natural assumption to make when the year is not explicitly mentioned. I told several members of the S.P.R. about the dream some time before the alleged date of fulfilment. Mr B. G. Mitchell of Keble College and Mr J. H. Hick of Oriel College were told orally ; and I wrote to Mr G. N. M. Tyrrell and Mr Denys Parsons about it in some detail.

Before I go on to describe what happened subsequently, it will be well to mention another dream which I had four years before, in September 1945. I regret that I have no note of the day of the month, but it was not very far from the date of the later dream (September 20th).

In this earlier dream I was aware of a rather indeterminate but dignified female figure, somewhat resembling a hospital nurse, who said, ' Does he know what is going to happen to him on December 1st? A journey and an illness.' These words were not addressed to me, and I was not aware of anyone else to whom they were addressed. But I was sure in the dream that they referred to me. Here again, either in the dream itself or immediately after

waking up, it seemed to me obvious that what was 'going to happen' to me was my death.

This dream had a peculiarly vivid and impressive character which I can still recall. It impressed me so much that as soon as I woke up I switched on the light and wrote down a few notes (a thing I have never done before or since); and I wrote to Mr G. N. M. Tyrrell about it the following day. I do not know how to describe this peculiar vividness. It was not the terrifying vividness of a nightmare. The dream was not terrifying at all. The emotion it aroused, at the time and afterwards, was the kind of mildly anxious concern which any important announcement about one's future career might occasion. But somehow the dream had a feel of seriousness about it, almost of solemnity, which ordinary dreams do not have. The dream of 1949, on the other hand, did not have these qualities. If I may put it so, the 'matter'—the information conveyed—was equally impressive to the dreamer; but the 'manner' had nothing peculiarly impressive about it. So far as the 'manner' was concerned, it was just an ordinary dream; rather clearer in its details than most dreams, perhaps, but not otherwise striking.

It will be noticed that the two dreams are curiously parallel. Both were predictions of death, or at least were immediately interpreted by the dreamer as being so. Both occurred at about the same time of the year. Both referred to a definite date in the future, December 1st, though in the second one this was conveyed in an oblique manner—'three weeks before the shortest day'. The year was not mentioned in either. But in both, in accordance with the ordinary idiom of the English language, I assumed that the date referred to was December 1st in the year in which the dream itself occurred. Both aroused the same emotion of mildly anxious concern, though neither was terrifying.

Two other relevant points must be mentioned before I go on to describe what happened afterwards. First, both in 1945 and in 1949, I was in unusually good health at the time of dreaming, having just returned from my annual holiday. Secondly, the reader will wish to know what likelihood there was that I should be ill about the beginning of December in any case. In 1949 it *was* likely. During the winters of 1947 and 1948, though not previously, I had had several illnesses of an influenza-like type, some of which continued for three weeks or more; but though troublesome and debilitating, they had shown no signs of being fatal. In 1945, however, it was much less likely that I should have anything more than the normal sort of influenza, lasting four or five days, which most people suffer from at one time or another during the winter months.

I need hardly say that neither dream was literally fulfilled. But in both cases the subsequent happenings were of some interest. Early in November 1945 I had an attack of pneumonia for the first time in my life. It was not very acute, but I had to give up work for a fortnight and was in a pretty weak state for some time after that. During this period of convalescence, about the end of November or the beginning of December 1945 (I greatly regret that I cannot fix the exact date), I took a walk one afternoon. It was a very fine day, and I walked rather farther than was wise. When I had gone about a mile, through the University parks and across the river, I suddenly began to feel extremely weak and ill. People sometimes say hyperbolically, ' I felt as if I was going to die '. In my case this was literally true. It was exactly how I did feel ; and I had never had such a feeling before, not even when I was injured in an aeroplane accident many years ago. The experience was not alarming, though it was certainly very surprising. In itself it was neither pleasant nor unpleasant, though as a philosopher I was naturally rather interested in it, in a detached sort of way. With great difficulty and very slowly I managed to get back to my rooms in New College ; and then it occurred to me that this walk might be the journey referred to in the dream I had had some two and a half months before. ' A journey and an illness ' *had* resulted, not indeed in my death, but in a very striking death-like experience.

The sequels of the 1949 dream were somewhat similar, though more complicated. I confess, with some shame, that I took the dream fairly seriously, and it was very much in my mind during the months of October and November. For example, I thought it worth while to write a long letter to my literary executors, to be sent to them in case of my death. Various little incidents also occurred which suggested to me, at the time, that the dream was going to be fulfilled. For example, one afternoon, at tea in Senior Common Room, a colleague suddenly began talking about ' a Phenomenalistic funeral '. Would there *really* be anyone in the coffin, he asked, since no one could look inside and see? (A Phenomenalist is a philosopher who holds that propositions about unobserved objects are to be analysed into sets of propositions about possible sense-experiences.) I am not a Phenomenalist myself, but critics have sometimes accused me of being so, and I have written and lectured a good deal on the subject, as this colleague knew. It therefore seemed to me odd that this particular academic joke should be made in my presence at that particular time, when the thought of my own death and particularly of my funeral was very much in my mind. I had not told anyone about the dream except the four members of the S.P.R. already men-

tioned ; and the last people I should have been inclined to tell were my New College colleagues, who would certainly have received the disclosure with a mixture of pity and ridicule. It seemed to me then, and still does, that an unpremeditated joke is quite a likely vehicle for the expression of unconsciously-received paranormal data, and has at any rate some analogy with a dream or a piece of automatic writing ; and I am inclined to think that whatever it was that caused my own dream of September 1949 was also a factor in causing my colleague's joke, by means of a ' telepathic leakage ' from myself. I had the same impression on another occasion when I came into Senior Common Room one evening, and a guest, whom I did not know, said in a loud voice, ' Call no man happy till he is dead '.

On November 25th I had to go to Stoke-on-Trent for an important meeting, and it was necessary to return the same evening. It was a long and fatiguing journey and resulted in an attack of my usual influenza-like disease (' a journey and an illness ' again?) from which I was still suffering on December 1st, the date mentioned in the dream. It was the longest attack I have ever had. I was eventually obliged to go into a nursing home in order to have a minor operation, and did not return to work until January 22nd, 1950, a week after the beginning of the Oxford Hilary Term. It may or may not be relevant that the operation took place on January 10th, which is nearly three weeks *after* the shortest day.

The illness itself was not serious, though very prolonged and very inconvenient ; but it was probably a contributary factor in causing a curious incident which occurred at my home on the evening of December 23rd or 24th. (It seems that December 23rd was actually ' the shortest day ', being a minute shorter than December 22nd, the day of the Winter Solstice.) The weather was very calm at the time. The furnace at my home which heats the radiators is old, and its chimney is not very efficient. As there was no wind, fumes began to come through into the room above, in which I was sitting. When I got out of my chair to fetch something, I suddenly found myself lying on my back on the floor, half under the table, in a very extraordinary state of physical weakness, and quite unable to get up again. This lasted for a minute or two, after which I managed to get up by holding on to the furniture ; but shortly afterwards I found myself lying on the floor again. Having managed to get up, I thought it would be well to go to my bedroom upstairs ; and when I had got there, with considerable difficulty, I collapsed on to the floor for the third time, upsetting a large pile of books and papers which fell in a cascade on my head. Fortunately the fumes did not penetrate upstairs, and after lying

on the floor for what seemed a very long time, I managed to stagger to my feet again, switch on the light, and go to bed. During these curious experiences, as in the one which followed the 1945 dream, I certainly felt as if I was going to die very shortly ; and I remember saying to myself ' So I *am* going to die, and the dream was right after all '. Like the 1945 experience, they were rather disconcerting than alarming ; and like it, they interested me so much that I should be sorry to have missed them.

The reader will have to judge for himself whether the experiences which followed the two dreams were or were not partial fulfilments of them. There certainly was a curious parallelism between the dreams themselves, and an equally curious parallelism between the ' death-like ' experiences which followed them, though in the 1949 case the experiences happened on or just after the shortest day and not three weeks before, as had been predicted.

It is of course a very natural hypothesis that a ' death-wish ' had something to do with causing the two dreams. It may be pointed out that a normal individual (if there is such a being) would have been greatly alarmed by a dream which predicted or appeared to predict his own death in the fairly near future, whereas I was only mildly concerned about it ; and that a normal man would also have been alarmed by the subsequent experiences I have described. If the reader knew me personally, he could easily assure himself that in the ordinary way I am extremely timid, and he would plausibly conclude that there must be some pathological cause to account for my lack of fear in this case.

On this ' death-wish ' hypothesis, I have two things to say. First, I am perfectly conscious of having such a wish from time to time. This is not because of, but in spite of, the belief in survival which I am also inclined to hold. The death-wish, in my case, is not at all a wish for a Blessed Hereafter. It is a wish for complete and permanent repose, in fact for an endless and dreamless sleep ; and it fits in well with the strong association (already mentioned) between the ideas of dying and falling asleep, and the ideas of being buried and being put to bed or ' laid to rest '. If mere wishes could decide the question, I should be inclined to wish that other people would survive death, since they apparently have such a strong desire to do so, but that I myself should not. I have very little doubt that this death-wish was the reason why neither the dreams themselves, nor the curious experiences which followed, alarmed me particularly, as they would presumably have alarmed a normal healthy-minded person. I may add that the non-fulfilment of the predictions, though in one way a relief, was in another way rather disappointing to me.

On the other hand, whatever part this death-wish may have played, it cannot account for the fact that a precise day of the month, and the same day too, was referred to in both dreams. At any rate, I am conscious of no wish which would make me prefer to die, or be buried, on December 1st rather than any other day.

MISDIRECTION AND THE MIRACULOUS

THE desire for the miraculous is a factor not lightly to be reckoned with in psychical research. It is met with in many branches, and especially in spontaneous cases, where testimony is often bedevilled by it. It is the ally of the illusionist, who with the potent weapon of misdirection forces our senses to accept what our reason would reject.

The skilful stage 'thought-transference' performer works against an even more favourable background than other illusionists, owing to the wide acceptance of the existence of telepathy.[1] The Piddingtons, first in Australia and recently in this country, have exploited this to the full, adapting their technique to the medium of broadcasting as Dunninger did before them in the United States. Naturally enough—for it is their livelihood—they chose to surround their performance with an air of mystery, giving full rein to the listener's imagination with their slogan, 'You are the judge'. This policy has proved to be extremely effective, and they have thrived on the arguments and discussions which it has provoked. Misdirection has been employed on a grand scale. Attention is concentrated on the 'telepathic' aspect of the performance, and references to Zener cards, to 'tests' etc. help to create the atmosphere of an experiment. Everything, in fact, is done to discourage the audience from considering what scope there is for the tricks of the conjurer. And in view of the public's unfamiliarity with the methods of the illusionist, and its ignorance of the conditions in which experiments are carried out in parapsychology, it is not surprising that it has been misdirected with a vengeance.

It is clearly to the advantage of a 'thought-transference' act that any mystification or argument which it may have aroused should be encouraged. This, no doubt, is why the Piddingtons' friend and manager Mr Russell Braddon, in a recently published

[1] For evidence in support of this, see *Mass-Observation Bulletin*, vol. 1, no. 12 (June 1949). Of those to whom the question 'Do you believe in telepathy' was put, 65 per cent replied 'Yes', 8 per cent 'No', 22 per cent 'Don't know', and 5 per cent 'Not interested'. The survey was carried out before the Piddingtons' first B.B.C. broadcast.

book,[1] does not conceal his displeasure with those who, including the Editor of this *Journal* and others with experience of parapsychology, have made it known that they do not believe that anything other than normal methods are used. No-one would wish to begrudge the Piddingtons their success as vaudeville artistes. Their performances have undoubtedly given entertainment to millions. But those who, clearly without knowledge of conjuring or of parapsychology, have attributed their effects to psychic faculties might profitably consider the following points: If two people were able to bring into play at will a faculty enabling them to transfer from one to the other words, symbols, and even whole sentences other than by means of the recognised channels of sense, why should they not (*a*) announce the fact to the world in precise terms, and (*b*) as human beings unique in history voluntarily submit themselves to scientific investigation?

After reports had appeared in certain organs of the Press giving the impression that the Piddingtons were claiming to produce their results by means of telepathy, the Hon. Secretary of the S.P.R., at the Council's request, wrote to Mr Piddington asking whether he and his wife were in fact making this claim. In the reply received from Mr Piddington no claim to paranormal powers was made.

As there appears to be some confusion as to the Society's attitude towards the testing of persons popularly credited with paranormal powers, this may be a suitable opportunity for stating the position. The Council are always prepared to charge the appropriate officers of the Society with the investigation, in conditions of their choice, of any person claiming to possess psychic faculties. It is not their practice to issue direct challenges to performers who are popularly supposed to possess such faculties but who make no claims themselves.

E. O.

REVIEWS

THIS WORLD AND THAT: AN ANALYTICAL STUDY OF PSYCHIC COMMUNICATION. By Phoebe D. Payne and Laurence J. Bendit. London, Faber, 1950. 194 pp. 10s. 6d.

Both the authors of this book are well known to investigators of *psi* phenomena, and the title of this book suggests the wide scope

[1] *The Piddingtons*, London, Werner Laurie, 1950. 238 pp. Illus. 8s. 6d.

of its interest. Two previous books by them—*The Psychic Sense* by Phoebe Payne, and *Paranormal Cognition* by Dr Bendit—will be familiar to many readers, and in this latest book the authors collaborate.

In personal forewords, both writers affirm the value and the indispensable need for psycho-analysis in their dealings with the psychic world. (In the title of the book ' This World ' is, of course, the physical world, and ' That World ' the psychic world.) Both authors are sensitive to paranormal experiences. Phoebe Payne describes how she felt that ' psychological knowledge was imperative if one were to find one's way through the maze of difficulties which mediumship and psychic phenomena present to a clear-minded person '. Dr Bendit underwent a ' training analysis ' in qualifying as medical psychologist.

The seriousness of Phoebe Payne's and Dr Bendit's approach is clear. Written (very readably) for the general and not the specialist reader, its aim is to teach the necessity for the knowledge of unconscious motives in studying the *psi* faculty, to call attention to the fact that unsolved problems in analysis are caused by *psi* not being recognised, and to stress the importance of the *psi* faculty being integrated with the personality. The authors are at pains to avoid being didactic, and to present alternative theories fairly. They leave open the questions touched on, and invite the reader to form his own conclusions.

The major part of the book is written by Dr Bendit, and he discusses almost the whole field of phenomena studied in psychical research. He does not allude to any scientific evidence, deciding that scientific evidence has already proved sufficiently for his purpose the existence of a *psi* faculty. If we accept this attitude in the main, it will enable the critical reader to forgive Dr Bendit for assuming that scientific evidence has proved the existence of phenomena which are still being hotly debated (such as physical mediumship, physical phenomena in ' poltergeist ' cases, and so on); but the book has the great merit that it is in the van of research, the important and inevitable advance in psychic research in making use of analytical knowledge, and in psychology, in accepting the reality of *psi*.

Some chapter headings will give an idea of the varied matters discussed—' What is Man ', ' Do we Survive? ', ' Negative and Positive Psychism ', ' Ghosts and Haunts ', ' Spiritualism ', ' Healing and Healers ', to mention a few of them. In ' What is Man? ' we gather the general background of Dr Bendit's thought and philosophy, and he discusses the grounds which lead him to believe in the possibility of man's further spiritual evolution. In some places the thought is not quite consistent. For instance, in

' Do we Survive? ' he writes that ' it may be that *personal* immortality is indeed a myth. But *individual* immortality is one of the deepest truths of life '. In ' Communications ' he writes that ' there seems to be no theoretical reason why telepathy should not take place between dead and living people '. But we must allow to Dr Bendit the fluidity of opinion that he recommends in the reader.

In ' Ghosts and Haunts ' he relates various types of experiences familiar to psychical researchers, without corroborative evidence, but with the aim of suggesting some psychological explanations. We wish that here he could have gone further in discussing the unconscious factors which can distort observation, as he does so usefully in other chapters. He asks, dramatically confronting us with the problem of survival, ' Would Hamlet's father have appeared on the battlements if Hamlet or his friends had been replaced by an automatically controlled cine-camera? ' Thus challenged, we can only sadly acknowledge our ignorance, but at the same time register our disappointment that Dr Bendit does not explore further any psychological alternative to the survival hypothesis. For does not analysis often prove such apparitions to be dramatic projections created by the inner conflicts of the subject (and surely Hamlet is almost an archetype of conflict)? We need not go further than the S.P.R. *Journal* to find a most interesting analysis of an apparition. In the November–December number Mr Edward Osborn describes in ' The Woman in Brown ' the genesis and dispersal of an apparition, and we feel that Dr Bendit must have interesting things to tell us of such apparitions which he has studied in his therapeutic work.

In the chapter on ' Guides ' there is an original idea which I have not seen suggested elsewhere. Dr Bendit's proposition is that a ' guide ', which he thinks is a dramatic representation of a remote or repressed part of the medium's psyche, fulfils a useful function as buffer between medium and sitter, a shield protecting the medium's personality; for sitters will accept from guides criticisms which might not be accepted coming from the medium, and the medium is able to express ideas which might otherwise be repressed. But Dr Bendit does not take only a negative view of the role of guide, for even if the guide be an aspect of the medium's mind, it can represent a higher and finer aspect of that mind.

' Negative and Positive Psychism ' is an important chapter, for it is here that Dr Bendit states most emphatically his views as to the role of the *psi* faculty in man's structure. He stresses (as indeed he does throughout the book) the dangers of the uncontrolled *psi* faculty, and affirms its value when controlled and integrated with the personality. He defines as a ' positive psychic '

one whose capacities are under the control of the will. It is here probably that most queries will arise in the mind of the reader, and that we could wish that Dr Bendit had given us more information, for it seems to be the very essence of *psi*, as most of us experience it, that it is spontaneous, and cannot be made subject to the will. An experimenter calling Zener cards, for instance, cannot ' will ' success. It seems that the most that ' will ' can do is to provide the best conditions and hope for the best results. And does Dr Bendit hold that all experiences coming from the unconscious are equally experiences of *psi*? Is there a difference in kind and not only in quality between a *psi* impresion of a Zener card, an experience (in Jungian language) from the Collective Unconscious, and a mystical experience?

But the fact that this book prompts such interesting questions is a tribute to it. It will be of interest to many readers to whom the analytical angle is new, and it is to be hoped that the authors will take up their pens again, and let us know in more detail, from their wide experience in therapeutic work, about the integration of *psi* and its positive value.

INA JEPHSON

TRANCE MEDIUMSHIP : AN INTRODUCTORY STUDY OF MRS PIPER AND MRS LEONARD. By W. H. Salter. London, Society for Psychical Research, 1950. 44 pp. 1s. 6d.

Trance mediumship is a subject which arouses more controversy than most branches of psychical research, owing presumably to its association in many people's minds with fraud and deception, and also because the communications obtained through entranced mediums nearly always claim to originate from the dead. For this reason, its importance is apt to be overlooked, and this excellent pamphlet by Mr Salter giving a concise account of the two famous mediums, Mrs Piper and Mrs Leonard, is particularly welcome, and will do much to dispel ignorance regarding the phenomena produced by highly gifted mediums whose honesty and integrity were beyond question.

The cross-correspondences produced by a group of automatists including Mrs Piper were the first and most remarkable examples of an attempt, by means of independent references in the scripts of the automatists, to produce a coherent literary pattern which was intelligible only when the separate pieces were all put together. They present a problem which could apparently only be explained by telepathy of a most complicated and subtle kind from the living, or by communication from a discarnate mind acting with purpose, or by a mixture of both. Mr Salter's description of these, and of

the book tests produced by Mrs Leonard, should encourage readers to make a closer study of these remarkable phenomena and the problems involved. The origin of the mediums' 'Controls' and the 'word-association' experiments carried out by Lady Troubridge and later by Mr Whately Carington are discussed, and the need is stressed for the study of these problems in relation to the growing knowledge of the paranormal faculties of living persons and the light thrown on disassociated states by medical psychology.

Mrs Richmond contributes an interesting appendix on the psychological aspect of the relationship between medium and sitter, and Mr Drayton Thomas gives examples of 'personal control' during his sittings with Mrs Leonard. There is a short but useful reading list.

As Mr Salter rightly says, 'There is no short cut to the understanding of the problems of mediumship'; but this pamphlet should show the student what an opportunity for valuable research lies open to anyone who realises the importance of this type of phenomenon as a means of throwing fresh light on the mysteries of the human mind.

K. G.

Democracy and the Quaker Method. By Francis E. Pollard, Beatrice E. Pollard, and Robert S. W. Pollard. London, Bannisdale Press, 1949. 160 pp. 8s. 6d.

This short and unpretentious book by three distinguished political and educational workers, who are members of a well-known Quaker family and one of whom is a member of the S.P.R., is of great interest both to the citizen and the student of psychical research. It examines the technique of conference in the light of the procedure at Quaker Meetings, where it is sought, not to enforce the will of the majority upon a reluctant minority—a vote is never taken—but to elucidate the 'sense of the meeting' in a constructive and generally acceptable solution, which, the authors say, has often not previously been envisaged. They point out that such a solution, even of controversial problems, will frequently emerge after a period of group silence; and they conclude from this and a number of other occurrences that 'there is some direct evidence that telepathy is at work in Friends' Meetings and those who share in these experiences of group working will often realise from personal experience that telepathy is operative. We do not', they say, 'dogmatise as to the extent or manner of operation, but we suggest to psychical researchers and those

concerned with the psychology of groups that here in Friends' Meetings is a fruitful field for joint investigation by them '.

There is an appendix on telepathy, relating the work and opinions of Rhine, Carington, Tyrrell, and other distinguished students of psychical research to the experiences and phenomena at Quaker Meetings.

R. I. H.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH, Volume 43, No. 4, October 1949.

A paper by Emanuel K. Schwartz on ' The Study of Spontaneous Psi Experiences ' aims at encouraging the careful and detailed recording and reporting of every instance of *psi* experience. It is intended to ask the contributors ' to provide sufficient data from their life history to be used in personality study for the determination of the psycho-dynamics of the particular experience '. For analytical work of this kind, it is permissible and commendable to cast a wide net and include cases which are poorly evidenced. One wonders, however, whether the net is not being cast too wide, to include cases sent in by people with no critical sense, which simply shout for a normal explanation. ' I thought of a friend in Germany, also in the Army, who I imagined got into a fight with a German and killed him. This later proved to be true.' Again : ' It was in a dream. I entered a movie theatre, looked around, took a drink of water, bought some candy, and then sat down. Several weeks later these experiences actually happened to me.'

Gardner Murphy writes a generous appreciation of his visit to the S.P.R. last year when he was President of the Society, and there is a reprint of Dr D. J. West's article in the March–April 1949 issue of the S.P.R. *Journal* making suggestions for future research.

Dr Jan Ehrenwald gives a reasoned reply to the review of his book *Telepathy and Medical Psychology* contributed by Ronald Rose to the July 1949 A.S.P.R. *Journal*. Dr A. Tanagra contributes some diffuse notes on the Theory of Psychobolie about which he has written a book.

Volume 44, No. 1, January 1950.

Dr Gardner Murphy's Presidential address to the S.P.R. last June is reprinted in full. F. Bateman and S. G. Soal describe long-distance experiments in telepathy between Cambridge (England) and Richmond (England) and between London and Antwerp. The work was carried out during Soal's tenure of the

Perrott Studentship in Psychical Research.[1] The medium was Mrs Gloria Stewart. The Cambridge-Richmond experiment was a failure, but the London-Antwerp series was an unqualified success. The corrected value for P was less than 10^{-8}. Displacement scores were also totalled. The effect was most marked on (−2) postcognitive guesses, with $C.R. = (-)\ 2{\cdot}4$.

There are reviews of Whately Carington's *Matter, Mind and Meaning* by C. J. Ducasse, and of Phoebe Payne and Laurence J. Bendit's *The Psychic Sense* by Montague Ullman.

D. P.

CORRESPONDENCE

Experiments on Frederick Marion

Sir,—After reading the letters from Dr Thouless and Dr Wiesner (*Journal*, March 1950) I find no reason to change my conclusion that, so far as my own experiments are concerned, Marion failed to show any evidence that he possesses paranormal powers. This, I believe, was also the opinion of most of the other members of the group. When sensory cues were possible Marion succeeded in finding the hidden object or in recognising the playing card, but when the audience stood behind a curtain or the cards were sealed in envelopes or screened from his sight he failed. It is true that in certain types of experiment the number of trials was not as large as could have been desired, but as I indicated clearly in my report this was due entirely to the fact that Marion left England for a period of four years before we had time to complete these particular series.

I do not think Marion's failure could be attributed to any hostility or undue scepticism on the part of the experimenters. All of us wanted him to succeed, and our relations with him were friendly and sympathetic to the end. Marion remained on excellent terms with myself for several years after the publication of the report, until apparently he realised that I was not a good advertisement for his paranormal claims.

I find Dr Wiesner's attitude rather puzzling. He apparently maintains that my experiments are no argument against the theory that Marion uses telepathy in, say, finding hidden objects, but that they only show that ' in certain complex situations positive results tend to disappear '. But surely the important point is that they tended to disappear only in those situations in which the possibility of sensory cues and hints was completely ruled out. Nor

[1] It is hoped to give an account of these experiments in a later issue of the *Journal*.—Ed.

do I understand why Dr Wiesner calls these situations ' complex '. I can see nothing complex in making the audience stand behind a curtain.

I can give Dr Thouless some details concerning the experiments with Zener cards. In these tests Marion was allowed to hold a pack of Zener cards chosen at random from 40 packs for a few seconds beneath the table. I then reshuffled and cut the pack out of his sight and placed it in an envelope fastened by a rubber band. Marion wrote down very rapidly a list of 25 guesses which was then compared with the order of the cards in the envelope working from top to bottom. On the first occasion Marion made his rectangles resemble squares with rounded corners and frequently his circles could hardly be distinguished from his squares, so that in ticking off correct hits we had to appeal to him as to whether he intended a square or a circle. Under these circumstances we counted 114 correct hits in 400 trials which represents a deviation from expectation of more than four standard deviations.

But when I re-examined the record sheets in the train that evening I realised that on almost every sheet there were symbols interpreted as circles which I should have judged to be squares, and vice-versa. I therefore took the eight sheets to Mrs Goldney who, with the ' target ' columns hidden from her sight, made her own interpretation of all the squares and circles. The result confirmed my doubts since, with the revised list, the number of correct hits was reduced to 94—a result without significance. For this reason I did not include this first experiment in the totals of my 1940 report but I made a brief note of the circumstances.

In the subsequent experiments we insisted on Marion's identifying each symbol before the target pack was removed from the envelope. In all Marion made 4,450 clairvoyance guesses using the above method at eight sittings, but never again did he score significantly above expectation. He scored, in fact, 910 correct hits altogether which result is only +20 above expectation and entirely without significance.

It is now my conviction that stage ' telepathists ' are not very suitable subjects for serious experiments in telepathy. They are adepts at deception and depend on this mainly for their livelihood. If the investigator is too trusting he is liable to be hoodwinked, but if, on the other hand, he is vigilant and alert, the investigation may become a battle of wits between him and the performer. In this case the experimenter is perpetually on his toes, so to speak, guarding against possible deception, and this is not in the best interests of genuine study. But if there was such a battle of wits between Marion and myself, it was certainly not Marion who won.

S. G. SOAL

Tijdschrift voor Parapsychologie

Sir,—It has been suggested to me that a passage in my summary [1] of Dr M. Lietaert Peerbolte's article in the *Tijdschrift voor Parapsychologie* for July–September 1949 might be open to misconstruction. With your kind permission, I should like the following sentence to be regarded as cancelled :

' Founding his belief on observation of spontaneous conversions to " Cosmic Consciousness ", especially among the aged, Dr Peerbolte strongly opposes the " spiritistic " aspects likely to be associated with Professor Van Os's more individualistic theory.'

—and the following to be substituted :

' Founding his point of view mainly on psychoanalytic experiences, Dr Lietaert Peerbolte strongly opposes Van Os's suppositions concerning the individual's *post mortem* situation.'

May I also be permitted to elaborate the phrase ' libidinous ties (in the analytical sense) ' used in my summary of Dr Tenhaeff's paper on ' Telepathic Double Dreams ' by giving the author's example of the ' transference ' situation which can come about between physician and patient during psycho-analytical treatment.

J. C. M. Kruisinga

THE SOCIETY'S NEW PRESIDENT

Dr S. G. Soal has been elected President of the Society for Psychical Research in succession to Professor Gardner Murphy. Dr Soal, who is Senior Lecturer in Pure Mathematics at Queen Mary College, University of London, has been a member of the Council of the Society since 1928, and has made many notable investigations into the mental phenomena of psychical research. His investigation in the nineteen-twenties of the mediumship of Mrs Blanche Cooper is well known, and in recent years he has made distinguished contributions to quantitative inquiries into extra-sensory perception, notably in the case of Mr Basil Shackleton and Mrs Gloria Stewart.

In 1947 Dr Soal delivered the ninth Myers Memorial Lecture, the subject of his address being ' The Experimental Situation in Psychical Research '.

[1] *Jnl. S.P.R.*, vol. 35, no. 656 (January 1950) p. 196.

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